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The effect of prison on adult re-offending

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***Aim**: To examine the effect of prison on re-offending among offenders convicted of either non-aggravated assault or burglary.*

***Method**: The effect of prison on re-offending was examined by comparing time to re-conviction among 96 matched pairs of convicted burglars and 406 matched pairs of offenders convicted of non-aggravated assault. One member of each pair received a prison sentence, while the other received some form of non-custodial sanction. All offenders were matched on offence type, number of concurrent offences, prior prison experience, number of prior appearances in court and bail status at final appearance. Cox regression was used to control for age, age of first conviction, gender, race, plea, number of counts of the principal offence, legal representation and prior breach of a court order. In the case of non-aggravated assault an additional control was included: prior conviction for a violent offence.*

***Results**: Offenders who received a prison sentence were slightly more likely to re-offend than those who received a non-custodial penalty. The difference was just significant for non-aggravated assault but not significant for burglary.*

***Conclusion**: There is no evidence that prison deters offenders convicted of burglary or non-aggravated assault. There is some evidence that prison increases the risk of offending amongst offenders convicted of non-aggravated assault but further research with larger samples is needed to confirm the results.*

***Keywords**: Prison, deterrence, re-offending, matched pairs, Cox regression*

INTRODUCTION

In Australia, as in most countries, prison is the main line of defence against serious and/or persistent offending. It is a very expensive form of crime control. It costs more than $260 per day to keep an offender in an Australian prison (SCRGSP, 2009, p. 8.24). At the time of the last national census, more than 29,000 people were being held in full-time custody (Australian Bureau of Statistics, 2009). Net recurrent and capital expenditure on prisons in Australia currently exceeds $2.6 billion per annum. National expenditure per person in the population, based on net recurrent expenditure on corrective services, increased in real terms over the last five years, from $100 in 2003-04 to $115 in 2007-08 (SCRGSP 2009, p. 8.4).

Despite the money spent on it, little Australian research has been conducted into the effectiveness of prison as a crime control. There are four ways in which prison might help control crime. Firstly, people cannot re-offend in the community during the period of their incarceration. This is known as *incapacitation*. Secondly, offenders who are imprisoned can be placed on programs designed to reduce their motivation to offend. This is known as *rehabilitation*. Thirdly, the existence of custodial penalties might discourage people in general from offending. This is known as *general deterrence*. Fourthly, the imposition of a custodial penalty might reduce the risk of re-offending only among offenders who receive this kind of penalty. This is known as *specific deterrence*. This bulletin is only concerned with the specific deterrent effect of custodial penalties. The question it seeks to address is whether, other things being equal, adults who receive a custodial penalty are less likely to re-offend than adults who do not receive a custodial penalty.

The bulletin is structured as follows. The next section discusses the results of past research on the specific deterrent effect of imprisonment. Section three explains the current study. Section four presents the results. In the final section we discuss our findings, their limitations and the implications of the study for future research and policy.

This bulletin has been independently peer reviewed.

PAST RESEARCH

There have been numerous reviews of the evidence on deterrence over the last ten years (Nagin, 1998; Gendreau, Goggin, & Cullen, 1999; Doob & Webster, 2003; Villettaz, Killias & Zoder, 2006; Pratt et al., 2006; Marsh et al. 2009; Nagin et al., 2009) but only the Villettaz et al. (2006) and Nagin et al. (2009) reviews focussed on specific deterrence. As the Nagin et al. (2009) review is the most recent, the summary of the evidence that follows is based on Nagin et al. (2009). They summarise the evidence bearing on these theoretical perspectives under four different headings: experimental and quasi-experimental studies, matching studies, regression based studies and 'other' studies. We adopt the same framework here but discuss recent Australian studies under a separate heading.

EXPERIMENTAL STUDIES

The best way to determine whether custody exerts a specific deterrent effect would be to take a large group of adult offenders and randomly allocate half to a custodial sentence and the other half to some other sentencing option. Killias, Aebi & Ribeaud (2000) took advantage of a facility in Switzerland under which offenders sentenced to short (14 day) periods of imprisonment could opt to serve the sentence as a form of community service order. Swiss law at the time allowed for testing, on an experimental basis, of innovative forms of correctional treatment, including alternatives to imprisonment. The Directors of Corrections in the Swiss canton of Vaud agreed to conduct an experiment in which eligible offenders were randomly allocated to prison or community service. The justification given for this seemingly inequitable treatment of offenders was that the resources available to manage offenders on community service orders were strictly limited.

The treatment (community service) group (n = 84) was compared with the randomised control (prison) group (n = 39) and another comparison group of 36 offenders who had been chosen for community service by correctional staff prior to the experiment. Measures were taken for each group of the prevalence and frequency of police recorded offending (police contacts) and court convictions before the index court appearance (i.e. the appearance at which they were allocated to groups) and after that appearance. The follow-up period was two years. The prevalence of police contact and conviction declined post allocation for all three groups, as did the frequency of police contacts and court convictions. No difference was found between the three groups in relation to the changes in prevalence of police contact. When the frequency of police contact was examined, however, the control (custody) and comparison groups were found to have had slightly more contacts after the index court appearance whereas members of the treatment group had significantly fewer.

Nagin et al. (2009) reported that all five experimental studies they reviewed found at least one criminogenic effect of incarceration, most of which were statistically significant. Two of these studies (Killias, Abei & Ribeaud, 2000; Schneider, 1986) involved juvenile offending. Three reported at least one deterrent effect. However only one of these effects was statistically significant and that study failed to separate deterrence from incapacitation effects.

The most compelling experimental study of deterrence comes from a study by Green and Winik (2010), which is too recent to have been included in the Nagin et al. (2009) review. Green and Winik (2010) tracked 1,003 offenders convicted of drug-related offences were randomly assigned to one of nine judicial calendars between June 1, 2002 and May 9, 2003. Judges on these calendars meted out sentences that varied substantially in terms of prison and probation time. The offenders were followed up for four years, with re-arrest being used as a measure of re-offending. Green and Winik (2010) found no significant effect of sentence or probation length on the risk of re-arrest.

MATCHING STUDIES

Kraus (1974) provides a good example of a matching study and his study is especially interesting for our purposes because he used Australian data. Kraus (1974) drew the first 50 consecutive entries from each of seven categories of offence from the probation register of the NSW Department of Child Welfare. He then used the Child Welfare Department's 'Institutional Index' to match each one of the 350 probationers with a comparable offender who was committed to an institution during the same period (1962-63). The matching was done on date of birth, age at current sentence, type of current offence, age at time of first offence, number of previous offences, category of previous offences and number of previous committals to an institution. Offenders were followed up for five years. Recidivism was measured in terms of rate of offending and the number of episodes of imprisonment, both overall and in relation to specific types of offences. Recidivism was found to be higher after detention for all but two offences: 'behaviour problems' and 'take and use motor vehicle'. There was no difference between the two groups in relation to 'behaviour problems'. Offenders who had served time in detention were less likely to commit the offence 'take and use motor vehicle', post release, than offenders who had been sentenced to probation.

REGRESSION STUDIES

Regression methods are the most common technique used to control for the influence of extraneous factors in studies on the specific deterrent effect of penalties on recidivism. Spohn and

Holleran (2002) compared 735 convicted drug offenders given probation sentences with 301 drug offenders sentenced to prison. The study controlled for age, sex, race, employment and type of drug offender. Rather than restrict themselves to these controls, however, Spohn and Holleran (2002) first constructed a model of the factors that influenced whether or not an offender was given a custodial sentence. This model included a number of factors likely to influence the choice of sentence but not likely to affect the risk of recidivism. The predicted probability of a prison sentence obtained in this first stage of the analysis was then added to the controls included in the second stage of the analysis. The advantage of this strategy is that it purges the penalty variable of any association with other factors correlated with both penalty choice and recidivism. The results of their study suggested that offenders who were given a prison sentence were more likely to re-offend and took less time to re-offend.

Nagin et al. (2009) found 31 regression studies measuring the impact of custodial sentences on recidivism. Only 16 of these studies, however, controlled for age, race, sex, prior record and conviction offence type. The coefficient measuring the effect of prison was positive (suggesting a criminogenic effect) in 13 of these studies and 12 reported at least one significant positive effect. Only three reported at least one significant negative (deterrent) effect. Nagin et al. (2009; p. 42), however, expressed concern about the crudeness of the controls for age in these studies. As they pointed out, re-offending risk is extremely age-sensitive. Small differences in the age distributions of offenders who were given custodial and non-custodial sentences could easily produce spurious differences in measured rates of re-offending.

OTHER STUDIES

Nagin et al. (2009) found seven studies that did not fit into the experimental, matching or regression study categories. Four of these are identified as having methodological problems (e.g. failure to separate deterrence from incapacitation effects; inclusion of non-convicted offenders in the comparison group). The remaining two studies were conducted by Drago, Galbiati and Vertova (2007) and Helland and Taborrok (2007). These studies are rather special in that the offenders involved actually knew the penalty that would be imposed if they were reconvicted.

The Drago, Galbiati and Vertova (2007) study is best thought of as a natural experiment. These authors examined the effect of a new bill passed by the Italian Parliament in July 2006. The *Collective Clemency Bill* was designed to address the overcrowding in Italian prisons and provided for a three-year reduction in detention for all inmates who had committed a crime before 2 May 2006. This resulted in the release of all those with a residual prison sentence of less than three years (some 22,000 inmates). Crucially for this study, the Bill stated that if a former inmate recommitted a crime within five years following his release from prison, he would be required to serve the residual sentence suspended by the pardon in addition to the prison time incurred as a result of the new offence. The effect of the Bill was to create a situation where the sentence for any future offence depended only on when an offender entered custody for the last offence. When Drago et al. (2007) analysed the effect of this natural experiment, they found that each additional month in the expected sentence reduced the propensity to re-offend by 1.24 per cent. The effect depended however, on the time previously served in prison. The longer the time already spent in prison, the weaker the relationship between the residual sentence and recidivism.

Helland and Tabarrok (2007) examined the effect of California's 'Three strikes and you're out' sentencing legislation. Under this legislation, an offender with two 'strikes' (convictions from a prescribed list of serious offences) who is convicted of another felony faces a prison sentence of 25 years to life and cannot be released prior to serving 80 per cent of the 25-year term. An offender with only one conviction for a strikeable offence who commits another felony faces a doubling of the length of the last sentence and no prospect of release until 80 per cent of the sentence is served. The second penalty in practice is much less severe then the first.

Because the factors that determine whether a defendant ends up convicted of only one strikeable or two strikeable offences (strength of evidence, competence of prosecutor etc.) are effectively random in nature, Helland and Tabarrok (2007) argued that the only systematic difference between the two groups was the penalty hanging over them for their next offence. To estimate the deterrent effect of the three-strikes sentencing legislation, then, they compared the re-offending rate of offenders released after conviction for two strikeable offences with the re-offending rate of offenders released after two trials for strikeable offences but only one conviction for a strikeable offence. They found that California's three-strike legislation reduced felony arrests among 'two strike' offenders by 17-20 per cent. No such effect was found in States that did not have three-strike sentencing legislation. This pattern of results suggests a deterrent effect.

SUMMARY OF PREVIOUS STUDIES ON THE SPECIFIC DETERRENT EFFECT OF IMPRISONMENT

Nagin et al. (2009) observed that most studies on the specific deterrent effects of custodial sanctions find these sanctions have a criminogenic effect. Nonetheless, given the many shortcomings among studies they reviewed, they felt bound to conclude that 'the jury is still out on … [custody's] effect on re-offending'.

Villettaz et al. (2006) drew much the same conclusion. They restricted their review to studies that were experimental or quasi-experimental. In their review of the 27 studies published between 1961 and 2002 that met this requirement, only two obtained evidence favourable to the specific deterrent effect of imprisonment. These two studies, as we noted earlier, have some unusual features. Ten of the remainder found no effect of imprisonment, four found mixed effects of imprisonment (some statistically non-significant, some favourable to the criminogenic hypothesis) and 11 found evidence uniformly supportive of the criminogenic effect of imprisonment. Five of the studies that found either no effect or a criminogenic effect were randomised controlled trials.

RECENT AUSTRALIAN RESEARCH

Although Australian research on the deterrent effect of prison is fairly limited, a number of Australian studies on the specific deterrent effect of prison have been conducted since the research reported by Kraus (1974).

Cain (1996) examined 52,935 offenders convicted in the NSW Children's Court between January 1982 and June 1992 and who had reached the age of 18 by the end of 1994 (this ensured that each juvenile was followed up until the end of their juvenile criminal career). He used logistic regression to determine whether juveniles who received a custodial penalty were more likely to re-offend after controlling for a variety of other factors (e.g. age at first court appearance, gender, offence type, place of residence). The results of his study suggested they were more likely to reoffend; however, as he acknowledges, several key factors that should be controlled for (e.g. length of prior criminal record, race) were omitted from his analysis.

Weatherburn, Vignaendra and McGrath (2009) compared two groups of juveniles, one of which (n = 152) had been given a control order (sentence of detention) and the other of which (n = 243) had been given some form of non-custodial sanction. Although they found a number of factors related to re-offending, the only factor to remain significant in their regression analysis was the prior record of the offender. After adjusting for prior criminal record, they found no significant effect of detention on risk of re-offending. Unfortunately, the small sample size and the focus on juvenile offending limits the conclusions that can be drawn from this study about the deterrent effectiveness of prison on adult re-offending.

The only recent Australian study to examine the effect of imprisonment on the risk of re-offending by adult offenders is that reported by Lulham, Weatherburn and Bartels (2009). This study used propensity matching to compare 6,825 offenders given a supervised bond with 7,018 offenders given a full-time prison sentence, all of whom had been convicted by a New South Wales court between 2002 and 2004 (inclusive). All offenders were followed up for a minimum of three years. The dependent variable was time to first proven offence, adjusted for time spent in custody. Separate analyses were carried out for offenders who had previously served time in custody and offenders who received their first prison sentence. Prison was found to exert no effect on time to re-offend amongst those who had not previously served time in custody. Offenders who had previously served time in custody, however, actually re-offended more quickly if they received a prison sentence than if they received a suspended sentence. The main limitation of the study is that the comparison group consisted entirely of offenders who received a suspended prison sentence. It is therefore of limited value in judging the deterrent effectiveness of prison compared with non-custodial sanctions in general.

THE PRESENT STUDY

Although the research conducted to date provides few grounds for thinking that custodial sentences reduce the risk of re-offending by adult offenders (and may even increase it), the issue cannot be regarded as settled. There are two main reasons for this. Firstly, most studies of the deterrent effect of prison have been conducted in countries other than Australia. It is not safe to assume that the findings obtained in overseas research on the deterrent effect of prison apply with equal force in this country. The profile of those imprisoned; the conditions and duration of imprisonment; the investment in prisoner rehabilitation; and the economic and social environment into which prisoners are released almost certainly differ markedly from one country to another. These differences may well affect the relationship between prison and re-offending. Secondly, as we have just seen, existing Australian research on the specific deterrent effect of prison is both limited and open to question.

The aim of this bulletin, then, is to help fill the deficit of Australian research evidence on the topic of imprisonment and re-offending. The focus of the study is on the specific deterrent effect of prison on two offences: burglary and non-aggravated assault. These two offences have been chosen for examination for two reasons. Firstly, both are very common. This is important because the matching process used to control for extraneous factors (see below) results in the elimination of a large number of cases. Second, offenders convicted of these offences face a non-trivial but less than certain risk of imprisonment. In 2003-2004, 46 per cent of persons convicted of burglary were imprisoned. Non-aggravated assault holds a less intermediate position, with 7.4 of per cent of convictions resulting in imprisonment (NSW Bureau of Crime Statistics and Research 2010).

Rather than rely on standard regression methods or propensity matching to control selection bias, this study relies on a combination of direct matching on key variables and regression analysis. Exact matching is used as the first line of defence against selection bias for two reasons. Firstly, it guarantees comparability on the variables used in matching and allows one to test directly for comparability on variables not used in matching. This is not possible when using regression. Secondly, unlike regression techniques, matching makes no assumption about the functional form of the relationship between the dependent variable and the covariates (Rubin, 1997). We do not need to make any assumption, in other words, about the precise nature of the relationship between risk of re-offending and key control variables, such as prior criminal record.

METHODOLOGY

DATA

The data for the present study were drawn from the New South Wales (NSW) Bureau of Crime Statistics and Research re-offending database (ROD). Further information about ROD can be found in Hua and Fitzgerald (2006). The ROD contains a record of each person who has appeared in a NSW court since 1994. The parent dataset from which the matched cases were drawn consisted of 171,969 cases finalised in the NSW courts between 2003 and 2004 (inclusive). The matched samples (see below) drawn from this dataset were followed up for a minimum of five years or until their first proven offence, whichever came first.

DEPENDENT VARIABLE

The measure of re-offending is free time to first offence (*Freetime*) resulting in a conviction. The term 'free' in this context means time spent *out of custody* either to the first detected *offence* or to the end of the observation period. Information on *Freetime* was obtained from ROD but is routinely supplied to the Bureau by the NSW Department of Corrective Services (DCS). Cox regression modelling with an appropriate adjustment for censoring at the *Freetime* maximum for individuals who do not re-offend was used to measure the effect of imprisonment on recidivism. The mean *Freetime* for burglary was 2.4 years (SD = 9.3 months). The mean Freetime for non-aggravated assault was 2.6 years (SD = 7.7 months).

INDEPENDENT VARIABLES

The question of what controls to include in any analysis is a particular challenge for two reasons. Firstly, there is no generally accepted theory of recidivism to guide the selection of control variables. Secondly, many of the variables that have been shown to influence recidivism are often not available for inclusion in the analysis. In their discussion of this issue, Nagin et al. (2009) argued that the minimum necessary set of control variables comprises prior criminal record, conviction offence type, age, race and sex. For each individual convicted of either of these offences in 2003 or 2004, the data set includes the following variables beyond conviction offence type:

1. ***Prison*** — coded 1 if a full-time custodial sentence was given and 0 otherwise;
2. ***Age*** — in years;
3. ***Race*** — coded 1 if Indigenous and 0 otherwise;
4. ***Gender*** — coded 1 for males and 0 for females;
5. ***Agefirst*** — equal to age in years at first court appearance;
6. ***Count_gp*** — coded 1 for one count and 2 for one or more counts of the principal offence; (7) ***Concurr*** — equal to number of concurrent offences;
7. ***Priors*** — equal to number of prior court appearances (including juvenile appearances);
8. ***Priorpri*** — coded 1 for individuals who had previously been imprisoned and 0 otherwise;
9. ***Bail Refused*** — equal to 1 if not on bail at final appearance, 0 otherwise;
10. ***Priorviol*** — coded 1 for individuals who had previously been convicted of a violent offence and 0 otherwise;
11. ***Legalrep*** — coded 1 for individuals who were legally represented and 0 otherwise;
12. ***Plea_gp*** — coded 1 for individuals who pleaded guilty and 0 otherwise; and
13. ***Priorbreach*** — coded 1 for individuals who had a previous conviction for breaching a court order and 0 otherwise.

MATCHING STRATEGY

Two considerations governed the selection of independent variables on which cases were matched: (a) past research on factors known to be predictive of both sentence and recidivism and (b) exploratory analysis of the current dataset designed to find variables that to do a good job at balancing non-matched variables between cases where the offender went to prison and cases where the offender was given a non-prison sentence. Concerning the second point, the ideal approach to controlling for potential confounders is to exact match on all measured covariates. However, as a practical matter this is impossible. As the number of dimensions upon which one attempts to match

grows, the availability of matches declines, as does the sample size, thereby reducing statistical power. Therefore it is important to be strategic in the choice of variables to match upon.

Snowball and Weatherburn (2007) in a previous study using data from ROD found that in addition to conviction offence type, burglary and non-aggravated assault in the case of this analysis, three other variables were strong predictors of a prison penalty: a prior record of imprisonment (*Priorpri*), number of prior court appearances (*Priors*), and number of concurrent offences (*Concurr*). These variables are also predictive of recidivism. This study matches on these variables and on bail status (*Bail*) which is also strongly related to both the choice of penalty and recidivism.

The inclusion of *Bail* status among the matching variables deserves special note because of the protection it may provide from bias from unmeasured covariates. The analytic strategy used in this report assumes that after matching on the four variables listed above within crime type that there are no unmeasured covariates that jointly affect the sentencing decision and recidivism. Such unmeasured covariates include information available to the sentencing judge but which is unrecorded in our data. In NSW judges are empowered to include their judgments about the risk of reoffending in making bail determinations. For our purposes here this is very important because the bail status variable may be absorbing these potential sources of hidden bias.

The parent database was searched for pairs of cases, one imprisoned and one not imprisoned that satisfied the following matching requirements:

1. Both members of the pair involved either burglary or non-aggravated assault
2. Both had the same prior prison status (*Priorpri* = 0 or 1)
3. Both had the same number of prior appearances (*Prior_gp* = 0,1,2,3,4-9 or 10+)
4. Both had the same number of concurrent offences (*Concurr* = 0,1,2,3,4,5+)
5. Both had the same bail status (*Bail* = 0 or 1)
6. One member of the pair received a full-time prison sentence and the other did not

This process resulted in 96 matched pairs for burglary and 402 matched pairs for non-aggravated assault. Cox regression was used to control for the effects of non-matched variables in analysing the effect of prison on free time to the first proven offence. The covariates included in the Cox regressions (*Age, Gender, Race, Plea, Count_gp, Legalrep, Priorbreach* and *Agefirst*) were identical for burglary and non-aggravated assault except that one additional variable (*Priorviol*) was included in the Cox regression for non-aggravated assault. *Priorviol* was included in the regression analysis of non-aggravated assault because it was found to be a strong bi-variate predictor of prison and re-offending for this offence.

Three models were constructed for each offence. The first model regressed free time to first proven offence against the variable: *Prison*. The second included *Age*, *Gender* and *Race* as controls. The third included the remaining control variables, that is, *Plea*, *Count_gp*, *Legalrep*, *Priorbreach*, *Agefirst* and, in the case of non-aggravated assault, *Priorviol*. The variables *Age* and *Agefirst* were first transformed into categorical variables prior to inclusion in the analysis. This avoids having to assume there is a linear relationship between time to re-offend and these variables.

RESULTS

We begin by seeing how matching the two groups (for each offence) on *Priorpri, Prior_gp, Concurr* and *Bail* influences similarity between the two groups on the other control variables. For each crime type, Table 1 reports the mean values (in years) of the two continuous independent variables included in the analysis (*age* and *agefirst*) for the imprisoned and non-imprisoned before and after matching. Table 2 shows the distribution of the categorical variables included in the analysis for assault before and after matching. Table 3 shows the distribution of the categorical variables included in the analysis for burglary, before and after matching. Also reported are two measures of the balance between imprisoned and non-imprisoned for each of these variables. One is the *p*-value for an F test for the difference in mean *Age* and mean *Agefirst* between prison and non-prison groups before and after matching. The other is the *p*-value for a chi-square test of the distribution of categorical variable values between prison and non-prison groups before and after matching.

The F-test results for Table 1 show a significant difference between prison and non-prison groups for assault in terms of age (*Age*) and age at first conviction (*Agefirst*) before matching. These differences disappear after matching. There is a significant difference between prison and non-prison groups in Age for burglary before matching. This also disappears after matching. There is no significant difference for burglary between prison and non-prison groups in *Agefirst* before or after matching.

Table 2 shows that there are significant differences for assault between prison and non-prison groups for all categorical variables except *Plea_gp* before matching. The differences in relation to *Gender* and *Priorbreach* are no longer significant after matching but the differences in the other variables

Table 1: Mean age and mean agefirst for prison and non-prison groups before and after matching

	Age			Age first convicted		
Offence	Prison	Non-prison	F-test *p*-value	Prison	Non-prison	F-test *p*-value
Assault before matching (n = 18,611)	30.8	33.3	<0.001	23.4	26.5	<0.001
Assault after matching (n = 804)	30.9	32.1	0.588	23.8	23.7	0.860
Burglary before matching (n = 1709)	29.0	26.8	<0.001	21.8	21.6	0.598
Burglary after matching (n = 192)	28.1	28.0	0.952	21.2	21.0	0.848

Table 2: Distribution of categorical variables for assault before and after matching (prison v non-prison)

	Assault before matching (n = 18,611)			Assault after matching (n = 804)		
Independent variable value	Non-prison %	Prison %	Chi-square *p*-value	Non-prison %	Prison %	Chi-square *p*-value
Male	82.9	92.1	<0.001	90.0	92.0	0.194
Indigenous	12.3	36.0	<0.001	22.1	33.2	<0.001
Prior breach of order	13.3	55.8	<0.001	48.8	53.0	0.130
One count of principal offence	93.9	97.9	<0.001	92.8	98.0	<0.001
Legally represented	70.4	94.8	<0.001	82.1	95.3	<0.001
Pleaded not guilty	23.6	22.5	0.316	25.4	22.1	0.155
Prior conviction for violence	83.1	16.9	<0.001	48.3	51.7	0.035

Table 3 Distribution of categorical variables for burglary before and after matching (prison v non-prison)

	Burglary before matching (n = 1709)			Burglary after matching (n = 192)		
Independent variable value	Non-prison %	Prison %	Chi-square *p*-value	Non-prison %	Prison %	Chi-square *p*-value
Male	85.5	93.7	<0.001	84.4	90.6	0.138
Indigenous	13.1	21.7	<0.001	19.1	18.2	0.514
Prior breach of order	21.5	52.1	<0.001	43.8	45.8	0.442
One count of principal offence	6.0	3.8	0.009	93.8	97.9	0.139
Legally represented	88.1	96.6	<0.001	88.5	95.8	0.052
Pleaded not guilty	12.1	12.8	0.333	11.5	16.8	0.203

(*Race, Count_gp, Legalrep, Plea_gp*), though smaller, remain significant after matching. In relation to burglary (see Table 3), there are significant differences in all variables except *Plea_gp* prior to matching. After matching the differences are no longer statistically significant, although the difference in terms of *Legalrep* approaches significance ($p = 0.052$), with those who are legally represented being more likely to receive a custodial finding. This somewhat surprising result probably arises because those who are more at risk of a prison sentence are more likely to seek and obtain legal representation.

Table 4 shows the results of the Cox regression modelling for non-aggravated assault.

The entries in the columns marked 'hazard ratio' show the likelihood of re-conviction at any point in the follow-up for an offender with a specified characteristic compared with an offender who does not have that characteristic. The hazard ratio of 1.32 in Model 1, for example, indicates that, prior to controlling for other factors, an offender who receives a prison sentence is about 32 per cent more likely than an offender who receives a non-custodial sentence to be reconvicted in the follow-up period.

Table 4: Cox regression models for non-aggravated assault

	Model 1		Model 2		Model 3	
Variable	**Hazard ratio**	**Confidence interval**	**Hazard ratio**	**Confidence interval**	**Hazard ratio**	**Confidence interval**
Prison						
Prison v non-Prison	1.32*	1.09-1.55	1.25*	1.04-1.51	1.22*	1.000-1.48
Age						
22-30 v under 22			0.70*	0.54-0.90	0.61*	0.49-0.81
31-40 v under 22			0.55*	0.42-0.71	0.47*	0.29-0.75
Over 40 v under 22			0.53*	0.38-0.75	0.49*	0.27-0.91
Gender						
Male v female			1.38	0.98-1.93	1.24	0.88-1.74
Race						
Indigenous v non-Indigenous			1.46*	1.19-1.78	1.24*	1.01-1.53
Plea						
Guilty v not guilty					1.18	0.95-1.46
Count_gp						
Counts =1 v Counts =0					1.16	0.75-1.80
Legalrep						
Legal rep v no Legal rep					1.18	0.86-1.62
Priorbreach						
Prior breach v no prior breach					1.37*	1.12-1.67
Agefirst						
19-22 v 18 and under					0.97	0.71-1.31
23-28 v 18 and under					0.94	0.61-1.45
29+ v 18 and under					0.92	0.55-1.54
Priorviol						
Priorviol v no prior viol					1.75*	1.30-2.36
-2 log likelihood	6,220.18		5,555.75		5,387.39	

*Significant at $p < 0.05$

The asterisk in the table indicates that the effect is statistically significant. In Model 1, then, the likelihood of re-offending (at any point in the follow-up period) is significantly higher for those who go to prison. When demographic controls are added (Model 2), the hazard ratio shrinks from 1.3 to 1.25 but remains significant and positive. When the remaining control variables are added (Model 3), the hazard ratio falls from 1.25 to 1.22 and barely reaches significance (p = 0.05). It can also be seen from Model 3 that *Age, Race, Priorbreach* and *Priorviol* are all significant independent predictors of time to re-offend.

Table 5 shows the results of the Cox regression modelling for burglary. The pattern is similar for burglary but the effect of *Prison* on re-offending is not significant in any of the models. The addition of demographic controls (Model 2) reduces the hazard ratio for Prison from 1.31 to 1.29. The addition of the remaining control variables (Model 3) slightly increases hazard ratio for Prison (to 1.37) but it remains non-significant. None of the controls in the third model are significant. This is to be expected given that the matching process for burglary (cf. Tables 2 and 3) removed any significant difference between prison and non-prison groups in the covariates used in the Cox regression.

Table 5: Cox regression models for burglary

	Model 1		Model 2		Model 3	
Variable	**Hazard ratio**	**Confidence interval**	**Hazard ratio**	**Confidence interval**	**Hazard ratio**	**Confidence interval**
Prison						
Prison v non-Prison	1.31	0.97-1.93	1.29	0.89-1.85	1.37	0.95-1.99
Age						
22-30 v under 22			1.24	0.80-1.93	1.13	0.71-1.89
31-40 v under 22			1.1	0.66-1.84	1.31	0.53-3.24
Over 40 v under 22			0.78	0.38-1.59	0.46	0.13-1.61
Gender						
Male v female			0.86	0.52-1.42	0.72	0.42-1.23
Race						
Indigenous v non-Indigenous			0.96	0.61-1.53	0.96	0.59-1.56
Plea						
Guilty v not guilty					1.09	0.66-1.79
Count_gp						
Counts =1 v Counts =0					0.54	0.19-1.53
Legalrep						
Legal rep v no Legal rep					0.92	0.45-1.86
Priorbreach						
Prior breach v no prior breach					1.13	0.82-1.80
Agefirst						
19-22 v 18 and under					0.83	0.489-1.410
23-28 v 18 and under					0.48	0.20-1.15
29+ v 18 and under					1.36	0.45-4.06
-2 log likelihood	1,230.99		1,138.68		1,102.00	

*Significant at $p < 0.05$

DISCUSSION

The purpose of this study was to examine the effect of the experience of imprisonment on adult reoffending. Prior to matching, recidivism risks for offenders convicted of burglary or non-aggravated assault who are sent to prison are significantly higher than for those who are given a non-custodial sentence. After matching and statistical controls have been introduced, prison exerts no significant effect on the risk of recidivism for burglary. The effect of prison on those who were convicted of non-aggravated assault seems to have been to increase the risk of further offending. These findings are consistent with the results of overseas studies reviewed in the introduction to this bulletin, most of which either find no specific deterrent effect or a criminogenic effect.

Before commenting on the policy implications of the study, it is important to mention three important limitations. Firstly, although the coefficient measuring the effect of prison on non-aggravated assault remains significant after the introduction of controls (see Model 3 in Table 4), we cannot rule out the possibility that some unmeasured factor is responsible for the higher re-offending rate among those who were sent to prison. The same point applies to burglary. Secondly, the number of matched pairs on which the analysis of burglary is based (n = 96) is much smaller than the number of matched pairs used in the analysis of non-aggravated assault (n = 402). It is possible that a larger sample of cases might have rendered the coefficient on the variable measuring the effect of prison for burglary statistically significant, in which case prison would have been seen to increase the risk of further offending. The third point is that we only examined offenders who committed assault or burglary. The findings may not generalise to other types of offenders.

The consistency of the current findings with overseas evidence on the effects of prison on re-offending suggests that it would be unwise to imprison offenders when the only reason for doing do is a belief in the specific deterrent effect of prison. This argument applies with special force to short sentences (e.g. sentences of six months or less) which are hard to justify on the grounds of deterrence or incapacitation and which provide little opportunity for prison rehabilitation. As it happens, substantial fractions of the prison population in NSW (6.8 per cent), Victoria (11.4 per cent) and Queensland (7.2 per cent) are serving sentences of six months or less (Australian Bureau of Statistics 2009). On the available evidence (see Aos et al. 2006) it would be more cost effective to place many of these offenders on a community based program which combines intensive supervision with drug, alcohol and/or mental health treatment. Following a recommendation of the NSW Sentencing Council (2007), the NSW Government recently introduced a new form of intensive corrections order that seeks to achieve this goal. It will be interesting to see whether this program is more effective than prison in reducing the risk of further offending.

Although it is conventional for researchers to end every study by calling for more research, in the present case the need should be obvious to all. Despite the vast sums of taxpayers' money spent on prison every year, we know very little about its effect on re-offending and crime. We do not know whether the apparent criminogenic effect observed in many studies (including this one) is genuine effect or just an artefact of selection bias. We do not know whether prison deters some offenders while increasing the risk of re-offending for others (leaving the net effect small or non-significant). We do not know whether the imposition of tougher penalties exerts a general deterrent or not. We have very little objective information on which types of Australian correctional program are effective in reducing re-offending and which are not. Although long prison sentences are often justified on the grounds of incapacitation, only one study in Australia has ever measured the incapacitation effect of prison (Weatherburn et al. 2006) and that study was limited to burglary. We have, finally, very little information on what contribution, if any, rising imprisonment rates have made to the fall in Australian crime. The lack of information on these issues is a serious impediment to the development of effective policy.

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